MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. LXVI. DECEMBER 8, 1905. No. 2

W. H. MAW, Esq., PRESIDENT, in the Chair.

Charles Frederick Aspinwall, B.A., Chestergate, Macclesfield ;
Hubert Hayward Champion, Uppingham School, Rutland ;
Lieut. Alfred Henry Laurence Ferris, R.N.R., Duncarrick, Castlerock, Co. Derry, Ireland ; and
Edward MacFarlane, Under-Secretary for Lands and Chief Surveyor for New South Wales, Department of Lands, Sydney, Australia,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Robert Courtenay, B.A. (Dublin), Indian Civil Service (retired), 34 Wilmount Street, Woolwich (proposed by A. M. W. Downing) ;
Chetwode G. G. Crawley, A.B. (T.C.D.), Lieut. R.M. Artillery, R.N. Staff of Science Instructors, H.M.S. Vernon, Portsmouth (proposed by Isaac Molloy) ; and
John Milne Gardiner Shaw, Mem. Inst. Naval Architects, F.R. Met. Soc., c/o John Swire and Sons, 8 Billiter Square, London, E.C. (proposed by E. T. Whittaker).

Sixty-one presents were announced as having been received since the last meeting ; including amongst others :—

Major C. F. Close, Text book of surveying, presented by the author ; W. G. Hooper, Æther and gravitation, presented by

the author ; Observations of the total solar eclipse of 1900 and 1901, presented by the U.S. Naval Observatory.

Frank McClean, Photographs of the Spectrum of Nova *Persei*, presented by Mrs. McClean ; Photographs of the total eclipse of 1905 August 30 taken by the Hamburg Observatory Expedition, presented by Professor R. Schöer ; Further charts of the Astrographic Chart of the Heavens, presented by the Royal Observatory, Greenwich ; Series of transparencies from the negatives of the total solar eclipse of 1905 August, taken at Sfax, Tunis, presented by the Astronomer Royal ; Portrait of Sir W. Huggins (lithograph), drawn and presented by Mr. Washington Green.

Note on the Astronomical Value of Ancient Statements of Solar Eclipses. By Simon Newcomb.

In Mr. Cowell's interesting paper on "Ancient Solar Eclipses," in the October number of the *Monthly Notices*, he mentions my views as to the value of ancient records of these eclipses, but fails to state them with entire exactness. I therefore venture to present a brief statement of them.

1. In order that an ancient total eclipse may be utilised for astronomical purposes, the record must inspire a fair confidence that the eclipse was actually total at a well-defined or determinable place of observation.

2. There is only one ancient total eclipse which seems to satisfy this condition, and this is one in which the record of its interpretation is certainly in error. I refer to the eclipse seen by the army of Xerxes, near Sardis, about B.C. 478.

3. Next in order of confidence comes the eclipse of Agathocles, which there is reason to believe was total somewhere in the Hellespont ; but this is vague.

4. In no other case is the fact of the eclipse being total at a definite place distinctly stated. It has been tacitly assumed that when an eclipse is mentioned in the annals of an empire, or by a poet or historian, it was total at the capital where the record was made, or at the place where the poet or historian lived.

5. Confronted with the dilemma :—

Either an historian, poet, or chronicler recorded an eclipse in which the limit of totality was an unknown number of miles distant from the point of record ; or

The laws of motion of the Moon were different in former ages from what they are now. I choose the first horn of the dilemma.

6. The researches of Brown show that during the past two centuries the motions of the node and perigee of the Moon have coincided with the results of gravitational theory within a limit

of exactness of 1″ annually. The presumption seems to me fairly strong that this accord has existed for the last 3000 years.

7. In view of the fact that, with the partial exception already mentioned, no ancient chronicler seems ever to have attributed the slightest importance to the question whether an eclipse of the Sun was or was not total at a given place, results based on the assumption that eclipses were total at the several places of record cannot be entitled to any great weight.

8. The act of rejecting the results of gravitational theory in order to secure the best possible representation of these supposed eclipses is subject to judgments similar to those pronounced by the Congregation of Cardinals against the doctrines of Galileo.

Reply to Professor Newcomb's Note. By P. H. Cowell.

The paragraphs are numbered so as to refer to the corresponding paragraphs of Professor Newcomb's note.

1. I agree with Professor Newcomb, but I would add that *à posteriori* evidence (the agreement of the equations of condition) has some value.

2. In all eclipses used by me, with the single exception of the eclipse of −762, there are words in the record implying at least a near approach to totality.

−1062	fire in the midst of heaven
− 647	day turned into night
−430 and − 309	stars seen
+ 197	light nearly extinguished

In giving first place to the eclipse of Xerxes, Professor Newcomb considers the words of the record only: he does not consider the interval of time and place between the event and the historian, nor the character of the historian for accuracy, as inferred from his other writings.

3. My papers are not concerned with the question as to whether the eclipse, stated at the end of the first century A.D. to have been once upon a time total at the Hellespont, was the same as the eclipse of Agathocles.

4. The assumptions mentioned form a good working hypothesis. Only two eclipses would have been necessary if totality at a given place were certain. The concurrence of four additional eclipses is offered to make up for the uncertainty of the historical data.

5. Professor Newcomb is not confronted with this dilemma. Were it so, I should admit that the second horn must be

avoided at all costs. The choice lies between the first horn, as Professor Newcomb states it; or

An acceleration of the Earth's orbital motion must be accepted which will make the eclipses agree well with the most probable interpretation of the records.

6. I am basing my hypothesis of a secular acceleration for the Sun upon the acceptance, and not upon the rejection, of the theoretical position of the node.

7. There are contemporary records of very few eclipses. Great numbers of partial eclipses must have occurred. I attach great weight to the fact that a very simple assumption makes the six eclipses considered central at specified places. In my view we have heard of those eclipses, because they were striking phenomena worth recording. The lunar eclipses and the transits of *Mercury* are not very searching tests, but so far as they go they confirm the assumption referred to.

8. I have not rejected the results of gravitational theory. I at once acquiesced in the following argument as soon as it was presented to me: *

i. The motion of the node accords with theory now to well within 20″ per century, which is therefore the extreme value to be assigned to the action of unknown causes.

ii. Therefore the position of the node twenty-five centuries ago must be considered known to within 500″.

I presume Professor Newcomb does not expect me to consider such an argument as the following conclusive:—

i. No theoretical reason is known for a change in the Earth's mean motion.

ii. Therefore the Earth's mean motion must be constant.

To expect this would be equivalent to saying that theory must always precede observation.

Finally I would say that the question is one of evidence. There must be a degree of evidence that would be considered sufficient to establish the fact. I have produced a considerable amount of evidence, all pointing one way; and the conclusion seems to me highly probable.

On the Transits of Mercury, 1677–1881. By P. H. Cowell.

In this paper I examine the transits of *Mercury* to determine whether they support or contradict the supposition of a secular acceleration of 4″ in the Earth's orbital motion that I have deduced from ancient solar eclipses.

It is well known that the secular acceleration of the Moon cannot be determined from modern observations on account of the term of long period, on which theory is altogether silent;

* In my first paper on ancient solar eclipses I had overlooked Professor Brown's paper on the secular accelerations in the *Monthly Notices* of 1897.

while observation can only give a very rough approximation to its value.

In like manner the assumption that no unknown long-period terms exist in the motion of the Earth or *Mercury* necessarily underlies the present paper, and hence no secular accelerations could have in the first instance been deduced from a discussion of the transits. I show, however, that with the above suppositions the transits of *Mercury* do indicate a secular acceleration for the Earth, but not for *Mercury*. The amount here deduced is $2''\cdot5$ for the Earth, in fair accordance with the $4''$ deduced from ancient eclipses, considering the short extent of time covered by the transits of *Mercury*. For *Mercury* the secular acceleration obtained is $-0''\cdot5$. If this be treated as accidental, and put equal to zero in the equations of condition, the secular acceleration for the Earth goes up to $3''\cdot2$, in still better accordance with the result from ancient eclipses.

The phenomenon, accounted for to a great extent in this paper by the supposition of a secular acceleration for the Sun, is twice recognised by Professor Newcomb in his discussion of the transits of *Mercury* (*Astron. Papers*, vol. i. p. vi). On p. 450 he finds the corrections required by the tabular times to be closely represented by $+60^s\cdot T^2$, a quantity far too large to be attributed to tidal retardation of the Earth's rotation. Again, on p. 460 he finds that the phenomena can be numerically explained by supposing a long-period inequality in the Earth's rotation sufficient to account for three tenths of the errors of the Moon. That there is something to explain may therefore be taken on the authority of Professor Newcomb. My present point is that a secular acceleration of the Earth does explain the phenomenon with reasonable accuracy, and that the hypothesis is not an arbitrary one invented *ad hoc*, but an hypothesis to which I had already been led by other evidence.

The material I use consists of the internal contacts only. Professor Newcomb (*Astron. Papers*, vol. i. pp. 457–8) has given equations of conditions resulting from fourteen November transits and six May transits, and has assigned weights to each equation of condition. I accept his weights unaltered. I remove, however, from his equations of condition every unknown quantity but V for the November transits and W for the May transits. To do this I substitute $k = 0$ in his equations, and for N, M, S the values that he has obtained on p. 459.

$$N = -0''\cdot16 + 0''\cdot28T$$
$$M = +0\cdot15$$
$$S = -0\cdot04$$

where T is the time measured in centuries from 1820.

It will be seen in Professor Newcomb's paper that N involves a correction to the node and S to the semi-diameters, and that

consequently relatively to V, W (which involve corrections to the longitudes), N and S change sign between second and third contacts. When, therefore, both second and third contacts are employed the values of V, W are very little affected by possible errors in N, S. Again, M involves a correction to the mass of *Venus*. There is not much uncertainty in this correction, and relatively to V, W its effect, though the same at both second and third contacts, changes sign twice or thrice during the period under discussion. Consequently the values of V, W will be very little affected by assuming a value for M.

The quantity k that I put equal to zero was introduced by Professor Newcomb on the hypothesis that the errors of the Moon might be attributed to irregularities in the rotation of the Earth. His solution $k = +0{\cdot}295$ means that three tenths of the errors of the Moon appear, on solution of the equations of condition, to be attributable to this cause. The choice, however, obviously lies between $k = 0$ (uniform rotation, the explanation of the Moon's errors to be sought elsewhere), and $k = 1$ (Moon's errors entirely explained by irregularities of rotation). An intermediate value of k merely replaces a single difficulty by two, and Professor Newcomb regards $k = 0$ as more probable than $k = 1$.

With these modifications the equations of condition become

November Transits. Internal Contacts.

Date.	Contact.	Equation.	Weight.
1677	II.	$-0{\cdot}98V - 5''{\cdot}72 = 0$	Rej
	III.	$+0{\cdot}96V - 9{\cdot}46 = 0$	Rej
1677	II. and III.	$-0{\cdot}97V + 1{\cdot}88 = 0$	0·3
1697	III.	$+0{\cdot}75V + 0{\cdot}69 = 0$	0·3
1723	II.	$-0{\cdot}95V + 0{\cdot}97 = 0$	2·0
1736	II.	$-0{\cdot}51V + 0{\cdot}20 = 0$	1·0
	III.	$+0{\cdot}47V - 0{\cdot}65 = 0$	1·0
1743	II.	$-0{\cdot}81V + 0{\cdot}21 = 0$	1·0
	III.	$+0{\cdot}84V - 0{\cdot}18 = 0$	1·5
1769	II.	$-0{\cdot}90V - 0{\cdot}43 = 0$	1·0
	III.	$+0{\cdot}88V - 0{\cdot}53 = 0$	0·2
1782	II.	$-0{\cdot}23V + 0{\cdot}11 = 0$	3·0
	III.	$+0{\cdot}19V - 1{\cdot}13 = 0$	3·0
1789	II.	$-0{\cdot}87V + 0{\cdot}80 = 0$	2·0
	III.	$+0{\cdot}90V - 1{\cdot}07 = 0$	1·0
1802	III.	$+1{\cdot}00V - 0{\cdot}63 = 0$	3·0
1822	II.	$-0{\cdot}46V + 0{\cdot}26 = 0$	0·5
	III.	$+0{\cdot}51V + 0{\cdot}55 = 0$	1·0

Date.	Contact.	Equation.	Weight.
1848	II.	$-0\cdot99V-0''\cdot61=0$	5·0
	III.	$+0\cdot98V+0\cdot70=0$	0·3
1861	II.	$-0\cdot75V+0\cdot26=0$	0·7
	III.	$+0\cdot72V+0\cdot76=0$	5·0
1868	II.	$-0\cdot62V+5\cdot90=0$	0·5
	III.	$+0\cdot66V+1\cdot72=0$	6·0
1881	II.	$-0\cdot96V-4\cdot34=0$	3·0
	III.	$+0\cdot97V+3\cdot40=0$	3·0

May Transits. Internal Contacts.

Date.	Contact.	Equation.	Weight.
1740	II.	$-0\cdot29W-1''\cdot76=0$	Rej
1753	III.	$+0\cdot97W+0\cdot47=0$	3·0
1786	II.	$-0\cdot64W+\left\{\begin{matrix}3\cdot59\\1\cdot19\end{matrix}\right\}=0$	Rej
	III.	$+0\cdot73W+1\cdot60=0$	4·0
1799	II.	$-0\cdot95W+0\cdot00=0$	3·0
	III.	$+0\cdot90W-0\cdot28=0$	4·0
1832	II.	$-0\cdot83W+1\cdot47=0$	6·0
	III.	$+0\cdot89W-1\cdot16=0$	6·0
1845	II.	$-0\cdot84W+1\cdot55=0$	8·0
	III.	$+0\cdot77W-1\cdot76=0$	8·0
1878	II.	$-0\cdot94W+1\cdot24=0$	12·0
	III.	$+0\cdot97W-1\cdot34=0$	8·0

Professor Newcomb, who had no grounds for introducing terms proportional to the square of the time into his solution, obtains

$$V=-0''\cdot90-2''\cdot63T$$
$$W=+0\cdot84+1\cdot84T$$

I find

$$V=+0''\cdot28-3''\cdot61T-3''\cdot30T^2$$
$$W=+1\cdot12+2\cdot36T-2\cdot81T^2$$

T being measured from 1820 in units of a century.

The meanings of V and W are

$$V=1\cdot487\delta\lambda-0\cdot487\delta\pi-1\cdot137\delta e$$
$$-1\cdot018\delta\lambda'+1\cdot19e'\delta\pi'+1\cdot58\delta e'$$
$$W=0\cdot716\delta\lambda+0\cdot284\delta\pi+0\cdot896\delta e$$
$$-0\cdot97\delta\lambda'-1\cdot11e'\delta\pi'-1\cdot62\delta e'$$

where $\delta\lambda$, $\delta\pi$, δe represent the corrections required by the tabular (Le Verrier's tables) mean longitude perihelion and eccentricity of *Mercury*, and accented letters refer to the Earth.

V, W refer to November and May transits respectively. The signs of the coefficients of $\delta\pi$, δe, $\delta\pi'$, $\delta e'$ are naturally changed at the opposite side of the orbits. If we regard δe, $\delta\pi'$, $\delta e'$ as known, the equations of condition determine the two relative distances of the heliocentric positions of three points, *Mercury*, the perihelion of *Mercury*, and the Earth.

Eliminating $\delta\lambda'$ and $\delta\lambda$ successively from the values of V and W

$$\delta\lambda - 1{\cdot}06\delta\pi - 2{\cdot}79\delta e \quad + 3{\cdot}16e'\delta\pi' + 4{\cdot}41e'\delta e' = -1''{\cdot}19 - 8''{\cdot}19\mathrm{T} - 0''{\cdot}50\mathrm{T}^2$$

$$-1{\cdot}07\delta\pi - 2{\cdot}98\delta e + \delta\lambda' + 3{\cdot}48e'\delta\pi' + 4{\cdot}92e'\delta e' = -2''{\cdot}04 - 8''{\cdot}48\mathrm{T} + 2''{\cdot}53\mathrm{T}^2$$

On the further assumption that the secular term on the right hand of the second equation is to be attributed to $\delta\lambda'$, we have

$$\delta\lambda' = +2''{\cdot}53\mathrm{T}^2$$

with similar assumptions

$$\delta\lambda = -0''{\cdot}50\mathrm{T}^2$$

The first result is a fair confirmation of the result from solar eclipses. The second is probably accidental.

A still better accordance with the eclipse value of the secular acceleration of the Earth is obtained by treating the secular acceleration of *Mercury* as zero in the equations of condition. In this manner we obtain from the November transits, 1677–1881

$$\delta\lambda' = +3''{\cdot}27\mathrm{T}^2$$

from the May transits, 1753–1878

$$\delta\lambda' = +2{\cdot}90\mathrm{T}^2$$

Weighting the two values in the proportion 204^2 to 125^2, or 8 to 3, the squares of the extent of the observations in time, we get

$$\delta\lambda' = +3''{\cdot}17\mathrm{T}^2$$

The terms proportional to T are slightly different from those obtained by Professor Newcomb, and they may be worked up by his methods, and slightly different numerical results in consequence obtained. The numerical changes, however, are small in comparison with the arbitrary assumption that δe, $e'\delta\pi'$, $\delta e'$ contain no terms proportional to the time other than those indicated by theory, thus throwing the whole observed discordance upon

$\delta\pi$. Moreover the numerical changes are small compared with the large "observed *minus* theoretical" value of $\delta\pi$, the longitude of the perihelion of *Mercury*. The motion of *Mercury's* perihelion remains anomalous, and is not shown to be the geometrical effect of ignoring secular variations in the tables.

On the Present State of Lunar Nomenclature.

By S. A. Saunder, M.A.

Some apology is due from me for again occupying the time of the Society with a subject so familiar to all selenographers as the confusion now existing in lunar nomenclature, and the inadequacy of our present system for the growing needs of selenography; but, as some recent remarks of mine* have led the Council of this Society to take a course of action which it is hoped may lead to an authoritative reconsideration of the questions involved, I have thought that a fuller statement of the difficulties might be of interest to those whose work lies in other directions, and might also lead to some useful suggestions from those who, like myself, have found themselves hampered by the want of a recognised language in which to express the results of their labours.

Our present system may be said to date from the publication of Beer and Mädler's map in 1837. In this the principal formations, such as Tycho or Copernicus, have separate names allotted to them; the smaller mountains are designated by affixing a letter to the name of some neighbouring principal formation, as Mösting A, Thebit B. But at once difficulties begin to be felt. These smaller mountains are denoted on the map only by the letters A, B . . ., and it is often far from easy to determine to which of the adjacent names this letter should be attached. Mädler was generally careful to place the letter towards that side of the object which was nearer to the named formation, but even in his map it is sometimes difficult to determine the name, and in other maps the position of the letter is no guide at all. The only safe method is to read through all that has been said in the text of *Der Mond*, or of Neison's *Moon*, under each of these headings until a description is found which applies to the mountain under consideration. This may well occupy half an hour, and sometimes three or four times as long may be spent without any result, for there are some of these lettered formations to which I have been unable to find any allusion in the text. It is frequently hard enough to identify a crater at all in a crowded region, and this further demand constitutes a considerable tax upon one's time.

As an instance of the confusion which may arise from this

* *Memoirs R.A.S.* vol. lvii. pp. 47, 48.

difficulty I may cite the valuable set of measures of 150 standard points published by Dr. Franz in *Breslau Mitteilungen*, vol. i.; No. 65 in that list is called Hippalus A, and no other means are given for identifying it except its position and diameter. Now I have measured the same point—it is No. 124 in my catalogue (*Memoirs R.A.S.* vol. lvii. part i.)—and, as my position differs from Dr. Franz's by less than a second of arc, whilst the diameter of the ring is about 9 seconds, there can be no doubt as to our having measured the same point. I am therefore able to identify the point on a photograph, and thence on the map. But, on referring to the text, I conclude that Mädler wrote of this as Agatharchides A, and not Hippalus A. Mädler's Hippalus A is No. 73 on my list, at a distance of nearly a minute of arc and on the other side of Hippalus. Anyone using Dr. Franz's measures as he intended them to be used, either for making a map or for determining the positions of other points, would probably have applied the measure to a wrong point, and, as it would not be absurdly wrong, he might have done a good deal of work before he found even that there was an error, and then a good deal more might be necessary before the true source was located.

A difficulty of the same character sometimes arises from the inaccuracy of the maps. When a point has been observed on the Moon, or on a photograph, it is often extremely difficult to identify it on the maps. Franz mentions this difficulty with regard to several points on his list, and, amongst others, with regard to No. 115, which he calls Pons *c*. Here, again, I have measured the same point (it is No. 1131 in my catalogue), and have come to the conclusion that it is Mädler's Pons *b*. The same confusion as before is not improbable, and these are not the only cases in which it might arise.

I should be very sorry if my selection of these instances from Dr. Franz's catalogue gave rise to an impression that I was actuated by any spirit of fault-finding. Dr. Franz has done so much towards the foundation of an accurate selenography that it would be impossible to speak of his work otherwise than with the highest admiration and respect; but, this being the case, the fact that I find myself obliged deliberately to differ from him in what ought to be so simple a matter as the right names to be applied to several amongst 150 of the most conspicuous measurable points on the Moon will be more convincing than many pages of argument that there is a real necessity for rendering the recognised names more easily discoverable.

But the difficulties do not stop here. Since Mädler's time many selenographers have considered it desirable to add new names to the list. In Mädler's map there were, according to Neison, 427 principal names: 145 of these were new, the rest being taken from the works of older selenographers. In Neison's map, which was published in 1876, there were 513 names. Of the additions one each was due to Webb, Lecoutourier, and

Schmidt, four to Lee, fifty-eight to Birt, and fourteen to Neison, whilst two names which Mädler had been unable to identify were reinstated from Riccioli and five from Schröter. Neison says that sixty-seven of these were taken from the British Association Catalogue, but I have not been able to ascertain that this catalogue was ever published. In 1864 a Committee of the British Association was appointed for the purpose of mapping the surface of the Moon, and this Committee issued five reports in the years 1865–69; but the only thing they did for nomenclature was to suggest a series of symbols which have never been adopted, and which I certainly should not wish to see put forward again.

The subject did occupy the attention of the Selenographical Society, who suggested the partition of the Moon into regions round each of the named formations, and who issued special catalogues of a few of these regions. Such of these, however, as I have examined seem to be based entirely on Neison's map, and they will not stand a detailed comparison with the photographs.

Schmidt's map—by far the best we possess—was published in 1878, and his description of it contains about sixty new names.

Franz has recently * given new names and letters to fifty-three points, and others have made further additions from time to time.

There is no map or catalogue in existence giving a complete list of the names which have been used by even half a dozen of our greatest selenographers; whilst Elger, in his book on *The Moon*, published in 1895, almost entirely neglects the names added by Schmidt, although his map claims to be founded on Schmidt's and to show clearly every named formation.

Schmidt, whose knowledge of the lunar surface has probably never been equalled, mentions several names as in use by English selenographers, but representing formations he has been unable to identify. Some of these may be found in Neison or Elger, but others cannot, and it would require some searching to discover who the authors were and to what formations the names were applied.

It will be seen that the present nomenclature has grown, and is still growing, without any recognised control, whilst the names given by one of the greatest of selenographers have, in this country certainly, and I believe elsewhere, received no recognition.

It has resulted from this that in many instances different formations have received the same names. Each of the following has been allotted to two: Peters, Carrington, Beer, Argelander, Janssen, Leverrier, Faraday. There are also two Lockyers, though these are so close to one another that the confusion would seem to have arisen from an error in identification. Again, the

* *Breslau Mitteil.* vol. ii, 1903.

names of Argelander and Janssen have been given by Schmidt and Birt respectively to the same formation, as also those of Leverrier and Miller to another, and those of Hencke and Daniell to a third.

It is possible that careful search would reveal other instances of duplication. I mention these as having been noted in the course of my work.

The introduction of a new name has, in general, involved a change for those minor formations which are nearer to the newly named crater than to that by whose name they were previously known. These retain the old letters but take the new name. Thus, when the name Murchison was given by Birt, the adjacent crater Triesnecker A became Murchison A. This same crater, Murchison A, has been named Chladni by Schmidt, and is erroneously called Murchison by Franz.

When the name Flammarion was introduced Mösting A should, in accordance with precedent, have become Flammarion A. But, thanks to the researches of Dr. Franz, Mösting A is to the Moon what Greenwich is to the Earth, the one point from which others are measured, and such a change would be intolerable ; but we are left with the anomaly that Mösting A, which is nearly fifty miles from Mösting, is actually on the wall of Flammarion.

Both capital and small letters are used to denote the minor formations ; thus Ptolemæus A and Ptolemæus *a* are quite distinct, whilst in some cases a third form, a, is introduced. This is an obvious disadvantage, as great care is necessary to prevent errors in transcription. Mädler's rule was to use capital letters for measured formations, small letters for those which were unmeasured ; Roman letters for depressions, Greek for elevations. But he did not adhere rigidly to these rules himself, and others have entirely neglected them. The rule with regard to capital and small letters is now quite unworkable, for it would involve a change of name when a previously unmeasured formation is measured, and moreover it would frequently be found that the corresponding capital had been already appropriated. The similarity of the capitals A and B in the two alphabets is another cause of confusion, though here again Mädler usually indicates a difference. Finally different letters are sometimes applied to the same crater in different maps, or the same letter to different craters.

Enough has been said to give an idea of the difficulties under which selenography labours, and further, as individual formations are studied and mapped, the number of small craters to be named may be increased twenty- or thirty-fold. It is eminently desirable that their designations should be allotted upon some uniform system.

If a remedy is to be found which will meet with universal assent—and nothing short of this would be a remedy at all—it is obvious that it must be the work of an international committee.

Hence the Council of the Royal Astronomical Society has passed a resolution that the International Association of Academies should be asked to nominate a committee to report upon the present state of Lunar Nomenclature, and to suggest a course of action for the future.

Part of the work of such a committee would be tedious, but would present no other difficulty. A list of all the cases of ambiguous nomenclature might be drawn up, and a decision taken as to which names should remain. It ought, also, not to be very difficult to devise rules for future guidance, and, if it were thought advisable to leave opportunities for the introduction of new names, to provide some simple machinery for the official sanction and publication of such introduction.

But the real difficulty would be to devise a means by which the decisions of the committee may be made intelligible to the world in general; to devise means by which selenographers may be enabled to identify with comparative ease the formation corresponding to any given name, or the name allotted to any given formation.

The ideal plan would be to prepare an accurate map, but this would seem to be a counsel of perfection. Schmidt spent thirty-four years in the preparation of his map, and we want a better. It is true that the photographs would to a certain extent diminish the labour; but even with these it would involve many years of patient work, whilst the knowledge, skill, and opportunity required to bring it to a successful issue are such as could rarely be found united in any one person.

It remains, then, to consider whether the photographs cannot be used directly, and I venture to put forward the following suggestions for criticism, in the hope that they may indicate a direction along which a solution may be ultimately found :—

If a series of four or five of the best modern photographs, showing the Moon under different illuminations, were enlarged to a diameter of from 12 to 18 inches, and the enlargements developed with impressed réseaux, so that the lines should appear in the same positions in all copies, it might be possible to identify a sufficient number of formations upon these and to give their positions in terms of réseaux coordinates. Two catalogues might then be formed, one so arranged that the name corresponding to any given formation could be at once ascertained, and the other so that the formation bearing a given name could be found. It is well known that photographs do not show anything like the amount of detail that can be seen with even a moderate telescope, but they do show a great deal. Mr. Hardcastle has partly measured more than 1800 small craters on one of Mr. Ritchey's exquisite negatives, and this by no means exhausts the number that could be identified on this one plate alone. The only part in which I should anticipate serious difficulty would be in the regions near the east and west limbs, of which it is not easy to get

photographs showing much detail. Possibly these might require special treatment.

Formulæ might be given for each plate for converting the réseaux coordinates into absolute selenographical coordinates, but the former would be more convenient for identification and for reference.

There are other questions which would also engage the attention of such a committee as has been proposed. Professor Shaler, writing as a "geologist and geographer," * has expressed the opinion that the present system of nomenclature is crude and inadequate; that the analogies suggested are often misleading; and that many of the more important features, such as capes, have been left unnamed. He suggests the appointment of just such a committee as is now advocated to undertake possibly the revision, and certainly the extension, of the present system.

Professor W. H. Pickering, again, has urged † that the existing names have been unfortunately allotted; that in many cases large and important areas have received the names of men who have done little for selenography, or even for astronomy, whilst many men who should be really commemorated are represented by small and insignificant craters. It is probably too late to alter this, but the criticism might well be considered by the committee.

On a New Method of Determining the Moon's Position photographically. By E. B. H. Wade, M.A.

(*Communicated by Professor H. H. Turner.*)

(1) Several methods have been proposed for determining photographically the Moon's position, either with a view to finding ephemeris errors or, given these, the longitude of a place. Amongst these methods we may mention that of Major Hills, R.E.,‡ in which a rigidly mounted camera is used to photograph the trails of suitable stars preceding the Moon, after which instantaneous exposures are made on the Moon itself, and finally further exposures are made for star-trails following the Moon. Again Professor Turner § has indicated a method of obtaining photographic transits which would be quite applicable to Moon culminations. Finally the same author has described ‖ an extremely direct method in which the Moon is photographed amongst the stars immediately surrounding it. Over-exposure of the Moon's image relatively to the stars is avoided by

* "A Comparison of the Features of the Earth and the Moon," *Smithsonian Contributions to Knowledge*, No. 1438, pp. 75, 76.

† *Harvard Annals*, vol. li. pp. 14, 15.

‡ *Mem. R.A.S.* vol. liii.

§ *Monthly Notices*, lvii. p. 349.

‖ *Monthly Notices*, lxiv. p. 19.

drawing an opaque diaphragm furnished with a narrow slit across the Moon's image during the whole period of exposure.

With regard to the first method, although it has the merit of great simplicity, yet it is subject to the inconvenience that a complete set requires at least two hours' work. During the whole of this time the camera must remain immovable, and as the diurnal motion is uncompensated the exposures must be timed with extreme precision. As Professor Turner has pointed out,* the methods of exposure on the Moon and stars are dissimilar, and this might lead to systematic errors. In any case the error in the deduced right ascension of the Moon is as great as the error in the time of exposure on the Moon.

With regard to the second method, it should be of a very high order of accuracy; but we are not, so far as I am aware, in possession of any actual results. I myself made some experiments in this direction in 1902, but was only partly successful, as I was employing a makeshift apparatus.

With regard to the third, if fog be successfully overcome it should enjoy all the accuracy of astrographic measurements. The method about to be described is put forward as an alternative one, and it is hoped to institute comparative experiments between the two.

(2) By means of double-image arrangements of mirrors it is possible to view the Moon in optical contact or proximity with stars which are in fact remote from it, and which in consequence are projected on a dark background. I have been working at intervals since 1901 November on methods of this kind, which are in fact extensions of the well-known method of observing the "lunar distance," and I have constructed an apparatus for *visual* observations on this principle, which is not yet, however, ready for publication. The object of the present note is to describe a photographic method by means of which the Moon's image is printed in the midst of stars at a distance of about 15° from it, so that all trouble from fog is avoided, and quite faint stars may be registered with instruments of small dimensions. All that is needed is an ordinary cœlostat and photographic camera; but the mirror of the cœlostat, instead of being worked to a true plane, is worked to a prism whose two faces are inclined at an angle of $7\frac{1}{2}°$, and whose edge is parallel to the polar axis. When the camera is pointed at such an arrangement it can photograph two fields whose centres are distant from one another by 15° in right ascension. No apparatus not in ordinary use is necessary, except that a prism is substituted for the ordinary mirror.

(3) A camera is mounted at such a height that its optic axis passes horizontally through the centre of the mirror, so that one half of the object-glass is illuminated by one face of the prism and the other half by the remaining face. I arrange the circle of the cœlostat so that (for example) the preceding face of the

* *Monthly Notices*, lxiv. p. 19.

prism projects the Moon's image in the centre of the field of the camera, and hold a sheet of white cardboard in front of the object-glass, to prevent any light entering the camera, while I draw the shutter of the dark slide. A semi-oval patch of moonlight is seen on the cardboard. The shutter of the plate-holder having been withdrawn, I move the cardboard laterally until the object-glass is uncovered, except for that half which is concerned in forming the Moon's image. The semi-oval patch of moonlight on the cardboard makes this operation easy. The card is held in this position during two and a half minutes; during the whole of this time the plate is receiving light from stars reflected by the following face of the prism, but none from the Moon. The cardboard is now moved sharply, so as to uncover the whole object-glass for a fraction of a second, and then restored to its former position. The exposure through half the object-glass is continued for another two and a half minutes. On development the plate is found to have registered an instantaneous photograph of the Moon in the midst of star images which have been exposed for five minutes, and have right ascensions differing by about one hour from that of the Moon.

(4) In the actual experiments the mirror of the cœlostat had a diameter of only four inches. The best lens which I had at my disposal was taken from a 2-inch visual achromatic telescope by Dallmeyer. On June 18 (when the Moon's age was fifteen days) in each of two consecutive exposures I secured eleven stars, none of them brighter than the magnitude $6\frac{1}{2}$, *and there was no sign of fog.* The method would therefore appear to repay trial on a larger scale. Out of the eleven stars the following were identified :—

15 *Sagittarii.*	82 *Sagittarii.*
16 *Sagittarii.*	ρ^2 *Sagittarii.*
21 *Sagittarii.*	

On the 19th I obtained :—

Preceding the Moon.	Following the Moon.
ν *Sagittarii.*	ι *Capricorni.*
ρ *Sagittarii.*	30 *Capricorni.*
	31 *Capricorni.*

But I am not at the present moment sufficiently equipped with maps or catalogues to identify the remaining stars, which, however, are certainly fainter than magnitude $6\frac{1}{2}$. That they are really stars is certain; for, the clock of the cœlostat being imperfectly adjusted, each is represented by a short trail parallel to those of the identified stars. I have not as yet the necessary apparatus for making precise measures of these images.

(5) The question will naturally suggest itself: How far does precision depend on the adjustments of the cœlostat? The only

essential function of the cœlostat is to make the star images stand still during the five minutes of exposure. A fuller discussion of this point will be given later. For the present it is enough to say that if we take two photographs, in one of which the Moon's image is projected on a field of stars following it, and in the other on a field of stars preceding it, the small errors, due to adjustment, can be eliminated and the angle of the prism determined. Owing to the great constancy of the angle of a prism, it may be desirable to determine it once and for all from star images without the intervention of the Moon.

(6) I have in conclusion to tender my best thanks to Captain H. G. Lyons, R.E., Director-General of the Survey Department, for providing facilities for this work and sanctioning its publication.

Helwan Observatory, Egypt:
1905 *July* 5.

Note.—Since the above was written I have obtained many other successful negatives with the apparatus described. The most interesting of these were taken in August in order to compare the ephemeris errors deduced with those given by the observations made at Helwan, Aswan, Edfu, and Chartas on the phases of the total eclipse of 1905 August 30.

Reproduction photographique des réseaux astrophotographiques.
By Henry Bourget.

1. Les astronomes qui ont manié des réseaux tracés sur argent savent quelles précautions exige leur emploi et combien ils se détériorent rapidement. Les piqures qui se forment dans la couche d'argent finissent, malgré les retouches, par les rendre inutilisables. Il faut acheter un réseau neuf, l'étudier et dresser le tableau de ses corrections. C'est une dépense et une perte de temps. Il serait très désirable d'en avoir de plus résistants et d'un usage, pour ainsi dire, indéfini. Un autre avantage d'un pareil réseau serait la possibilité de l'imprimer sur les plaques, au châssis-presse, par contact parfait. Ce serait à la fois plus commode et plus précis que le mode d'impression actuellement employé.

Un pas important a été fait dans cette voie par M. Izarn quand il a décrit dans le Bulletin de la Carte du ciel le procédé permettant de recouvrir la couche d'argent d'une couche mince de gélatine bichromatée insolée et par suite dans un état presque

corné que l'on connait bien. C'est une excellente pratique, mais d'application un peu délicate.

Il m'a semblé qu'on rendrait service en cherchant à faire une copie photographique d'un réseau, assez précise pour permettre de l'utiliser à la place du réseau lui-même. Voilà plusieurs années déjà que je fais des essais dans cette direction. Je vais dire ce que j'ai obtenu.

2. Cette copie doit être, naturellement, de même sens que le réseau, c'est-à-dire à traits transparents sur fond noir. Il faut donc, pour cela, tirer une copie d'une première épreuve négative. Mais, d'autre part, il y a intérêt, pour diminuer les erreurs inévitables de copie, à réduire autant que possible le nombre des tirages. On est donc amené à essayer d'obtenir du premier coup une épreuve positive du réseau, en utilisant le renversement de l'image photographique par solarisation.

La partie délicate du procédé est d'obtenir une copie présentant le contraste maximum entre la transparence des traits et le noir du fond et d'éviter les tons gris uniformes si fréquents dans les images renversées.

Voici le procédé auquel je me suis arrêté, sur l'avis bienveillant de M. Izarn. Il a toujours réussi parfaitement, en observant les précautions que j'indique. On choisit une plaque sensible très rapide (Lumière Σ) sur glace de préférence. On l'expose au soleil 10 secondes fortement pressée derrière le réseau qu'on veut copier, dans un châssis-presse. On la développe ensuite dans un révélateur énergique, peu importe lequel du reste, tout bain fort réussissant bien. L'image commence par apparaître négative, puis se renverse, grise d'abord, le contraste entre les traits et le fond croissant graduellement. Il faut surveiller avec soin le développement et l'arrêter au moment où le contraste paraît le plus grand et avant que le plaque ne passe par une troisième état de grisaillement uniforme. On achève le traitement de la plaque comme à l'ordinaire.

Si l'on trouve que le fond, malgré le développement énergique, n'est pas assez noir, on peut renforcer la plaque. Voici un mode de renforçage qu'on peut répéter autant de fois que l'on veut et qui m'a bien réussi en cette circonstance. On commence par décrasser la plaque en la soumettant pendant quelques instants à la liqueur de Farmer [ferricyanure de potassium et hyposulfite de soude]. On lave, puis on la plonge dans le bain suivant :

Eau.	100 c.c.
Bichromate de potasse . . .	2 gr.
Acide chlorhydrique	1 c.c.

et on l'y laisse jusqu'à ce qu'elle soit également blanche sur les deux faces. On la retire alors, on lave abondamment et on la développe à nouveau *en pleine lumière.*

J'ai l'honneur de présenter à la Société astronomique Royale,

en même temps que cette note, un spécimen de copie faite en suivant ces prescriptions, du réseau 8 cm. × 8 cm. que j'emploie à Toulouse pour les clichés de Nébuleuses et d'amas, photographiées au grand réflecteur de l'observatoire.

Pour qu'on puisse apprécier le degré de précision obtenu, je donne les résultats de quelques mesures faites sur le réseau et sur une copie.

Intervalle des traits 11–14.		Intervalle des traits 40–43.	
Réseau.	Copie.	Réseau.	Copie.
7 tours, 507	7 tours, 504	7 tours, 498	7 tours, 495
502	500	497	501
506	501	496	499
501	501	496	500
500	500	500	502
Moyennes... 7 tours, 5032	7 tours, 5032	7 tours, 4992	7 tours, 4994

Le tour de vis vaut à peu près un millimètre. On voit que l'écart entre le réseau et la copie est négligeable pour le travail courant de la photographie astronomique.

Je signale, en terminant, un dernier avantage d'un telle copie. Sa valeur commerciale, pour ainsi dire, nulle, la rend accessible sans dépense à l'astronome amateur.

Toulouse : 1905 *novembre* 10.

Position of the Axis of Mars. By Professor Percival Lowell.

(*Communicated by A. C. D. Crommelin.*)

In 1905 the same course was pursued in the determination of the position of the Martian axis by measurement of the north polar cap as in the previous oppositions of 1903 and 1901. As the arctic spring haze lasted longer than in 1903, and the post-opposition phase invaded the north polar limb earlier, not so many observations could be secured. These began on April 7 and were continued to June 1, opposition having occurred on May 8. All the measures after this latter date were theoretically affected by the phase encroachment and those after May 17 sensibly so. The correction for this, so far as it is calculable from the difference between the tangent supposed taken on the limb and really taken along the phase ellipse, has been applied. The alteration due to lack of light there is no means of estimating.

Tabulated the measures follow :

Date.	M.S.T. h m	Position-angle. °	wt.	λ. °
April 7	16 24	128·2	4	297·5
9	14 55	128·3	3	257·7
12	16 8	128·9	4	248·5
13	14 39	127·3	2	217·8
15	14 18	125·5	3	194·8
	32	124·4	3	198·2
	37	126·1	2	199·4
17	14 0	127·3	2	172·5
	50	125·5	3	184·7
	16 0	126·9	3	201·8
19	14 31	124·3	3	162·2
	15 12	128·1	3	172·2
24	14 14	126·4	4	113·7
26	13 54	126·1	2	91·1
	14 30	127·0	2	99·9
30	13 48	126·1	2	54·3
May 5	11 40	126·9	3	339·1
	12 12	127·1	4	346·9
7	10 57	126·2	3	311·1
May 10	10 49	127·6	2	282·8
11	10 40	128·8	3	271·8
	11 56	129·7	4	290·4
12	10 36	129·9	4	262·1
	11 50	129·3	4	280·1
16	11 32	130·4	3	240·6
17	12 39	128·7	2	248·1
21	11 39	128·0	3	198·3
22	10 55	126·1	3	178·8
23	10 57	124·0	2	170·4
24	11 0	125·0	3	162·3
	20	125·7	5	167·2
	46	126·7	5	173·5
26	9 49	126·4	4	127·3
	10 38	125·9	3	139·2
30	11 18	129·5	4	113·5
	12 15	129·2	2	127·4

In the discussion three methods have been employed :

(1) *dp* has been found from the pre-opposition measures only ;
(2) from the measures to May 17 inclusive ;
(3) from all measures, those after opposition having had applied the correction to the tangential angle due to the phase ellipse.

The three resulting values of *dp* are :

(1) $-1°·64 \pm 0°·22$; (2) $-1°·49 \pm 0°·21$; (3) $-1°·73 \pm 0°·20$

With these several values of *dp*, P has been corrected for the corresponding epoch :

1	Epoch 1905 April 23		36°·66
2	„ 1905 April 28		36·99
3	„ 1905 May 4		36·97

and the resulting great circles successively compared with those from 1901 and 1903 to the following determinations of the position of the Martian axis :

		Axis. R.A.	Axis. Dec.	Tilt of Martian Equator to Martian Ecliptic.
		° ′	° ′	° ′
1	1905 Before Opposition and 1901	315 51	54 6	23 35
2	1905 Through May 17 and 1901	315 57	53 58	23 43
3	1905 All, Corrected and 1901 ...	315 47	54 11	23 29
1	1905 Before Opposition and 1903	317 9	54 17	23 59
2	1905 Through May 17 and 1903	317 28	54 9	24 11
3	1905 All, Corrected and 1903 ...	317 2	54 20	23 55
	And in addition: 1905 Before Opposition and 1903 before Opposition ...	317 18	54 18	24 2

There is a close accord *inter se* in both sets of 1, 2, and 3, showing that the corrections are not vital. Between the two sets, however, there is a difference difficult to explain.

	R.A.	Dec.	Tilt.
	° ′	° ′	° ′
The mean of 1901 and 1905 is:	315 52	54 5	23 36
that of 1903 and 1905:	317 13	54 15	24 2

For the position of the vernal equinox of the planet we have:

		R.A.	Dec.
		° ′	° ′
1	1905 Before Opposition and 1901	84 47	24 28
2	1905 Through May 17 and 1901	84 39	24 27
1	1905 Before Opposition and 1903	86 36	24 33
2	1905 Through May 17 and 1903	87 50	24 37

of which the means of the sets are respectively:

R.A.	Dec.		R.A.	Dec.
84° 43′	24° 27′	and	87° 13′	24° 35′

The data derived from 1901 and 1903 (Lowell Observatory Bulletin, No. 9) for epoch 1905 are:

	Axis. R.A.	Axis. Dec.	Tilt.	Vernal Equinox. R.A.	Vernal Equinox. Dec.
	° ′	° ′	° ′	° ′	° ′
1903 Expurgated and corrected for setting on phase ellipse and 1901	315 8	55 3	22 37	85 52	24 32
1903 Before and after Opposition, expurgated and corrected...	315 58	54 40	23 13	86 2	24 33

The means for all four sets, viz. 1901–1903, 1903 before and after opposition, 1901–1905, and 1903–1905, are:

Axis.		Tilt of Martian Equator to Martian Ecliptic.	Martian Vernal Equinox.	
R.A.	Dec.		R.A.	Dec.
316° 3′	54° 31′	23° 22′	85° 57′	24° 32′

The result nearest to this is Lohse's, derived from measures on the north polar cap in 1884, 1886, and 1888, of which the mean is :

Axis.		Tilt.
R.A.	Dec.	
316° 45′	54° 31′	23° 39′

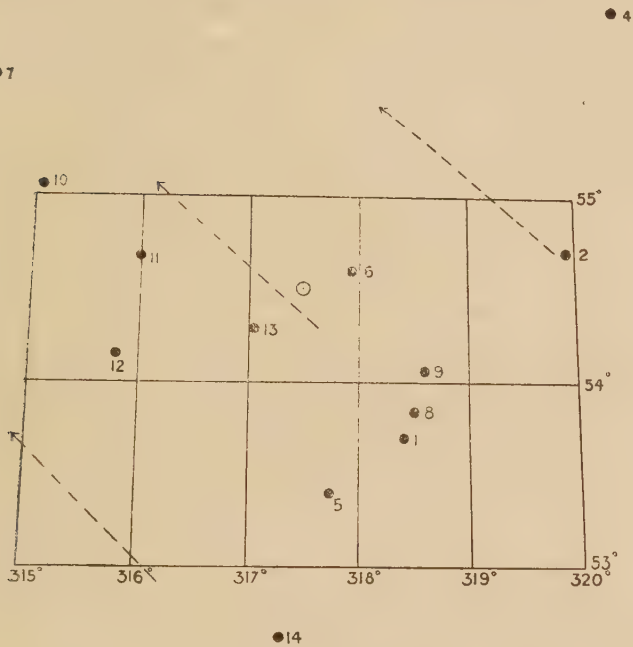
Schiaparelli's, Lohse's, and Cerulli's determinations have been completed and brought up to the epoch 1905 May 1 by taking account of the precession of the equinoxes. The correction for the Martian precession of the equinoxes has also been applied to the older determinations of the position of the axis of *Mars* to bring them down to date. This last as given by Struve, 7″·07 per annum, amounts only to six tenths of a minute in declination and three tenths in right ascension for twenty-six years. The results, together with Struve's and the writer's, are given in the following table :—

			Axis.		Tilt of Equator to Martian Ecliptic.
			R.A.	Dec.	
1	S.	1877–1879 (Schiaparelli) ...	318° 25′	53° 40′	24° 57′
2	N.	1882–1884 „ ...	319 52	54 42	24 50
3	N.	1882–1886 „ ...	320 56	55 17	24 52
4	N.	1884–1886 „ ...	320 22	56 0	24 9
5	N.	1884–1886 (Lohse)	317 43	53 24	24 51
6	N.	1884–1888 „	317 52	54 36	24 3
7	N.	1886–1888 „	314 39	55 34	22 4
8	S.	1892–1894 „	318 31	53 51	24 51
9	S.	1896–1898 (Cerulli)	318 38	54 4	24 45
10	N.	1901–1903 (Lowell)	315 8	55 3	22 37
11	N.	1903 Before and after Opposition (Lowell)	315 58	54 40	23 13
12	N.	1901–1905 Complete corrected (Lowell) ...	315 47	54 11	23 29
13	N.	1903–1905 Complete corrected (Lowell) ...	317 2	54 20	23 55
14		Struve (Satellites)	317 18	52 39	25 13

These several values have been plotted and appear in the following chart, the adopted mean position for the pole being denoted by the sign ⊙. The arrows indicate the direction of the pole of the Martian ecliptic.

Measures of the position of the cap in the drawings by the

writer in 1903 (Bulletin No. 18) show a correction to be needed to the ephemeris tilt, diminishing that tilt by 1°·7 ; thus making the value 23°·5 instead of 25°·2, as deduced and used in the ephemeris.



Position of the Pole of *Mars*.

The writer was therefore minded to see what Schiaparelli's drawings might have to say on the subject. He measured accordingly the drawings published by Schiaparelli in his fifth and sixth Memoirs, the only ones available for the purpose, and found that they placed the centre of the cap as follows :

1886.

Drawing No.	Lat. Centre of Cap.	Position of Pole as in Centre of Cap on the Disk.	Ephemeris Pole on the Disk.	Difference.	λ.
I.	67°·3	22°·6	21°·8	− 0°·8	Not given on drawings,
II.	66·4	23·6	21·8	− 1·8	
III.	66·8	23·2	21·9	− 1·3	
IV.	68·2	21·8	21·9	+ 0·1	
				− 3·8	
		Mean		− 0·9	

1888.

Drawing No.	Lat. Centre of Cap.	Position of Pole as in Centre of Cap on the Disk.	Ephemeris Pole on the Disk.	Difference.	λ.
	°	°	°	°	
VII.	70·4	19·6	24·7	+ 5·1	10
VIII.	66·7	23·3	24·8	1·5	350
IX.	68·9	21·1	24·8	3·7	340
X.	66·7	23·3	24·8	1·5	320
XI.	67·9	22·1	24·8	2·7	300
XII.	70·6	19·4	24·8	5·4	300
XIII.	67·1	22·9	24·9	2·0	240
XIV.	67·8	22·2	24·9	2·7	220
				24·6	
		Mean		+ 3·1	

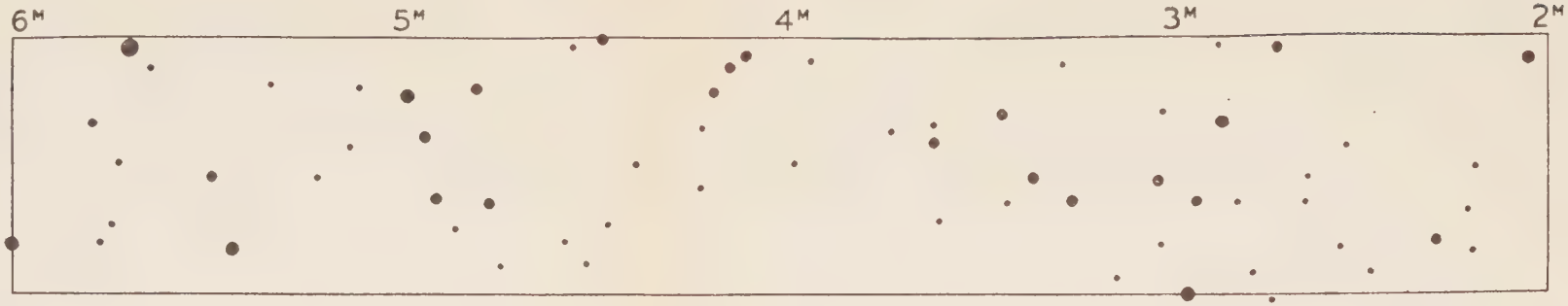
It might be argued that the drawings of the latter opposition are the better as with each opposition a draughtsman grows more proficient. But assuming all to be of equal weight, we have from the drawings of both oppositions an excess of tilt of ephemeris over observation of 1°·7, supposing the centre of the cap upon the pole. Now, in his *Memoria IV.*, Schiaparelli gives for the position of this centre a displacement of 2°·69 in λ 323°·5, and, in his *Memoria V.*, of 1°·269 in λ 295°·1. Comparing the longitudes of the drawings in 1888, we see that in most of them the centre was on the south side of the pole, which would increase the apparent polar tilt, not decrease it. The irradiation would do likewise. That the drawings indicate a tilt less than the ephemeris in spite of both these factors strengthens the conclusion that that tilt is too large. Indeed, in the future the tilt is more likely to be yet further reduced than increased.

From all these determinations the most probable position of the pole of the Martian equator seemed to the writer, after consultation with Mr. Crommelin, to be :—

R.A. 317°·5 and Dec. 54°·5. Epoch 1905. Tilt of Martian Equator to Martian Ecliptic 23° 59′.

The above value has been adopted for physical ephemerides of the planet in the British *Nautical Almanac*, beginning with that for the opposition of 1909.

REGION following δ Ophiuchi. R.A. 16h. 9m, Dec. – 3° 27′.



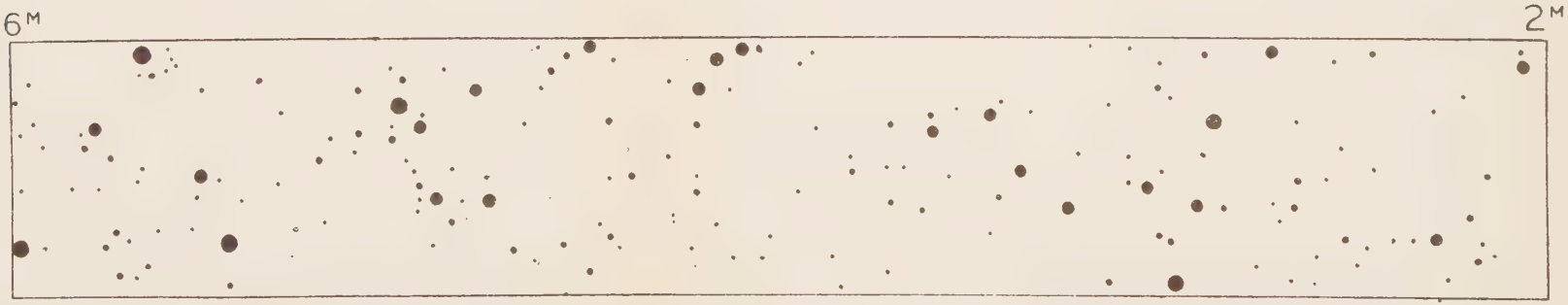
Mag.		Number of Stars.
8–9	...	1
10–11	...	5
12–13	...	19
14–15	...	38
		63

Chart of the U.S. Naval Observatory, Washington, with 26-inch glass.
E. S. HOLDEN, del.

Mag.		Number of Stars.
8–9	...	1
10–11	...	5
12–13	...	19
14–15	...	42
16–17	...	94
		161

Chart of the Lick Observatory, Mount Hamilton, with 36-inch glass.
R. H. TUCKER, Jun., del., 1894.

3.



Mag.		Number of Stars.
8–9	...	1
10–11	...	5
12–13	...	19
14–15	...	46
16–17	...	101
		172

Chart of the Lowell Observatory, Flagstaff, Arizona, with 24-inch glass.
PERCIVAL LOWELL, del., 1905.

Comparative Charts of the Region following δ Ophiuchi.
By Percival Lowell.

In an examination of the region following δ *Ophiuchi* Mr. Lampland found that the faintest stars visible through the 36-inch glass of the Lick Observatory as recorded in Tucker's map of the region, and consisting of seventeenth-magnitude stars, were perfectly visible at Flagstaff with the 24-inch glass of this observatory. To test the point thoroughly charts of the region were accordingly made by him and by the writer independently. Rainy weather (thunderstorms) and moonlight somewhat interfered with both observers, especially with Mr. Lampland, who had the least good hours. The charts of the two substantially agreed, and the writer's is here given. Together with it charts of the same region have, by permission, been reproduced: the one, by Dr. Holden, I believe, made with the 26-inch glass of the Naval Observatory at Washington about twenty years ago; the other, the one mentioned above, by Mr. R. W. Tucker at the Lick Observatory in 1894.

The stars shown in the three maps are respectively :—

United States Naval Observatory	...	63 stars
Lick Observatory	...	161 „
Lowell „	...	172 „

Their separation into magnitudes will be found under the several charts.

The writer's chart was made independently of Mr. Tucker's—except that the latter's positions for the brighter stars was followed, which was the course Tucker himself had pursued with the Washington one—and then was afterwards compared with his. It does not pretend to be exhaustive, as it was made in the bad season, but will suffice for purposes of comparison.

As the difference in light between consecutive magnitudes is 2·5 times the lesser, and the light-gathering power of 36 inches is 2·25 times that of 24 inches, the difference in aperture should, other things being equal, have meant nearly a magnitude lower for the stars shown by the larger glass. As the Flagstaff glass actually revealed more stars than the Lick, these charts would indicate that the difference in the clarity and definition of the air of the two sites amounted to about one magnitude. In this connection it is interesting to note the respective altitudes of the several stations :—

Washington	...	50 feet	above sea-level.
Mount Hamilton	...	4200 „	„
Flagstaff	...	7250 „	„

Though undoubtedly a factor, height was probably aided by steadiness; since the steadier a star, within certain limits, the more impression it would make on the retina.

On the Conditions determining the Formation of Cloud-spheres and Photospheres. By Arthur W. Clayden, M.A.

In the course of an investigation of the conditions under which clouds may be formed in our own atmosphere certain considerations presented themselves which seem equally applicable to the conditions which determine the position of a stellar photosphere in the mass of a star.

As the spectrum of a star is to a great extent dependent upon the position of the photosphere it seems possible that a survey of these points may help to clear up some of the difficulties attending the interpretation of spectral details.

To begin with it is necessary to ask that it may be taken for granted—

1. That the photosphere of a star is the upper surface of a stratum of clouds.

2. That those clouds are caused by the condensation of some substance from the state of vapour to that of small solid or liquid particles.

3. That the condensation is due to cooling produced by expansion brought about by the ascent of vapour-charged convection currents.

4. That the cooling effect of expansion follows the same general thermodynamic law as is the case in our own atmosphere.

It is true that under the high pressures and temperatures of a star the gradation of temperature may be considerably modified. The transference of heat from one stratum to a higher by conduction and radiation should tend to equalise temperatures, but the increased viscosity due to pressure should tend in the opposite direction. Hence a curve showing the relations of temperature and pressure is not likely to differ very greatly from one plotted in accordance with the expression

$$\frac{\log t - \log t'}{\log p - \log p'} = \frac{\gamma - 1}{\gamma}$$

in which t is the absolute temperature at a pressure p, and t' is the absolute temperature at the reduced pressure p' ; γ is, of course, the ratio of the two specific heats.

In order to argue from the known to the unknown, let us first consider the case of planetary bodies surrounded by an atmosphere consisting wholly of water, a substance whose temperature-pressure relations are well understood.

At temperatures far below freezing-point ice gives off vapour which exerts a certain maximum pressure. As the temperature rises this maximum pressure increases more and more rapidly. This goes on until 365° C. is reached, at which point the maximum pressure is 200·5 atmospheres. If at any temperature the pressure be less than the maximum, evaporation will take place ;

and conversely if the pressure exceed the maximum, condensation will follow until the pressure is reduced to that value.

We can then plot a curve showing the maximum pressures for all temperatures. Let this be done, taking temperatures as abscissæ and pressures as ordinates.

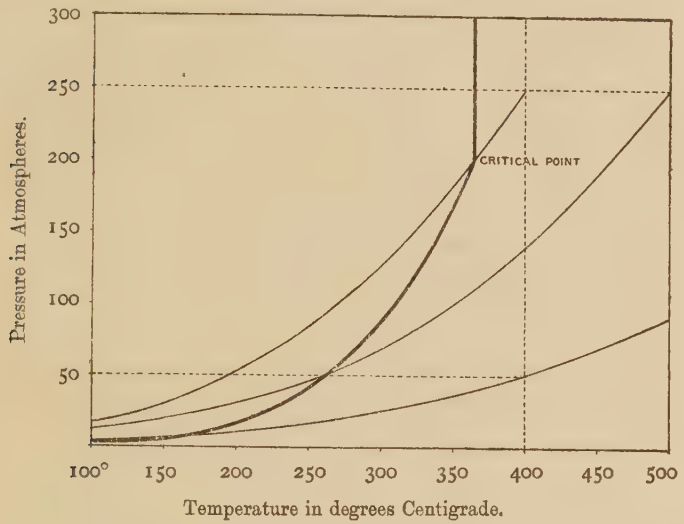


Fig. 1.—Condensation Curve for Water Vapour.

We find the curve rises at first very slowly, and at 100° C. it shows a pressure of one atmosphere. It then turns upwards more and more rapidly until it reaches 365° C. and 200·5 atmospheres. This is the critical point; and if the temperature be higher no increase of pressure can possibly bring about liquefaction. The curve may be regarded as giving the pressures and temperatures of condensation. If, then, the pressure is greater than 200·5 atmospheres, condensation will be effected at the critical temperature. The curve must therefore turn vertically upwards.

Let us now assume that the masses of atmosphere and planet are such that the pressure on the solid surface is 250 atmospheres, and that this surface has a temperature of 400° C. No liquid water can exist on such a surface, but condensation will occur at a certain height.

If the curve of decreasing temperature be plotted, using the expression quoted and giving to γ the proper value for water vapour—namely, 1·3—it will be found that this curve will cut the condensation line very near the critical point, at which temperature and pressure cloud-production will begin.

Suppose, next, that the surface temperature is increased to

500° C. If a similar curve be now plotted it will be found to cut the condensation line at about 50 atmospheres and a temperature of only 260° C. ; that is to say, the effect of increasing the surface temperature of the planet is, not only to drive the cloud-level further up in the atmosphere, but to lower the temperature at which its formation begins.

Again, suppose the surface temperature to be 400° C. but the pressure only 50 atmospheres. The curve then cuts the condensation line at about 160° C. and a pressure of less than 10 atmospheres.

It thus appears that the result of suitably diminishing the mass of a planet may be to produce exactly the same effect upon any cloud-sphere by which it is surrounded as would be brought about by increasing its temperature ; or that a hot planet of large mass might present exactly the same features as a cooler and smaller one.

If we imagine the planetary atmosphere to contain other non-condensible gases this will not affect the conclusions. The changes due to alterations of temperature and pressure will still be in the same direction, and the diagram will serve equally well if we remember that it relates only to the pressures and temperatures of the water vapour present. The actual pressures in the whole atmosphere could be computed by calculating them for the other substances and adding those values.

An inspection of the diagram shows that no cloud-sphere could possibly have a higher base temperature than 365° C. This is one point worth noting.

Next it is evident that if our eyes were so constituted that we could see the radiations emitted from the outer surfaces of such cloud-spheres they would become true photospheres ; and if the distances were great enough we should see these supposed planetary bodies as star-points. If two of them had their cloud-spheres at similar positions in their atmospheres they should present similar spectral features. We should then class them together, although they might really owe their apparent similarity to their true diversity.

Finally it is obvious that a determination of the temperature of the outer surface of the cloud-sphere would be no measure whatever of the temperature of the solid planet beneath.

It is not necessary to point out that these conclusions have an important bearing on the cloud-spheres surrounding the actual planets.

It seems only reasonable to attempt to apply them also to the stars.

Now it is not necessary to make any assumption as to the nature of the substance which makes up the cloud-particles of a stellar photosphere, nor is it necessary to assume that all photospheres are due to the same body. Whatever the substance concerned its condensation-curve would probably present features

similar to those of all bodies for which the requisite data are obtainable.

The argument, however, will be clearer if we start with the assumptions that all photospheres are due to the same substance, and that that is the element carbon. It will be easy later on briefly to consider other possibilities.

We have no measurements of the vapour-pressures of carbon. But there seems some reason to think, from the phenomena of the electric arc, that the vaporisation temperature under one atmosphere pressure is about 3770° absolute. This, then, will correspond to 373° absolute, the boiling-point of water under one atmosphere.

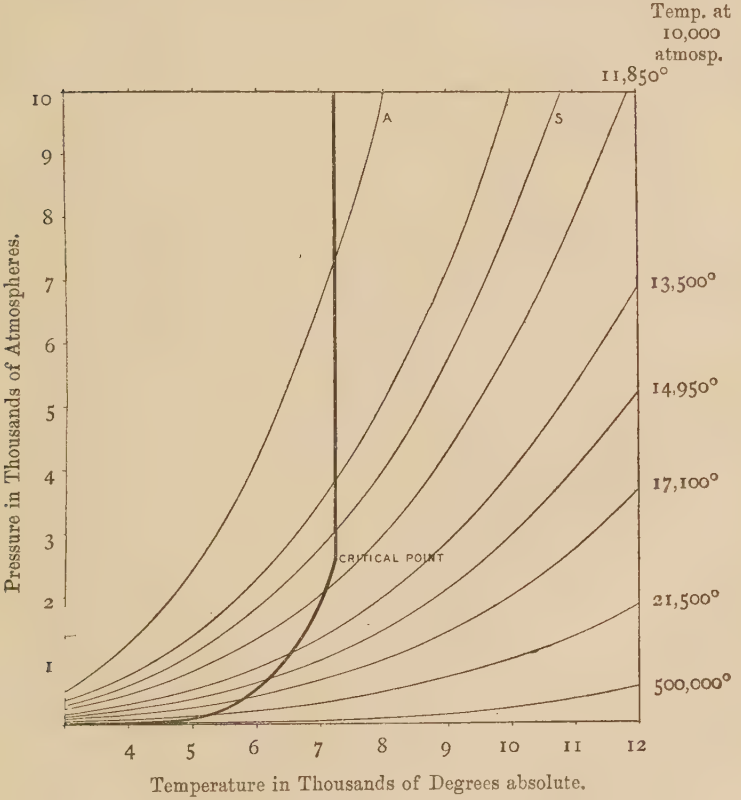


Fig. 2.—Hypothetical Curves for Carbon Vapour.

If we can get some idea as to the critical temperature we can then draw a curve resembling the known curve for water, and feel tolerably sure that our proceedings are reasonable. Messrs.

Wilson and Gray estimated the temperature of the solar photosphere at 6900° C. ; but this is the upper surface of the cloud stratum, which must be cooler than the base. Hence if we take 7200° absolute as the critical temperature we can feel sure that we are at least within the mark, and may be a long way within it.

The difference between 3770 and 7200 is about thirteen times as great as that between 373 and the critical temperature for water. Hence if the condensation-curve for carbon is similar to that for water the critical pressure should be about 2600 atmospheres. If we connect the two points thus fixed by a curve resembling that for water it will serve our purpose, since neither the exact temperatures nor pressures are material to the issue, which rests only on the supposition that the vapour-pressures and temperatures for the photospheric substance can be represented by some curve of the usual type.

Let us draw the curve, and again, for simplicity, consider only the pressures due to the carbon vapour.

Before drawing the curves to represent the changes of temperature and pressure in the stellar mass we are confronted with another unknown—namely, the value to be assigned to γ. According to the determinations of this quantity yet made, it appears that elementary bodies whose molecules are monatomic give a value 1·66; those whose molecules are diatomic give 1·4 to 1·3 ; while polyatomic molecules give values decreasing with the complexity, but always greater than unity. At stellar temperatures it seems unlikely that there will be polyatomic molecules, so that if we take 1·5 as a working value we shall not be far wrong. Moreover it will soon be evident that a change in the value of γ will in no way affect our general conclusions, but will only modify the numerical examples which serve the purpose of making the argument clearer.

Let us now imagine six stars in which the pressure of 10,000 atmospheres is reached at 8000°, 10,000°, 11,850°, 14,950°, 21,500°, and 500,000° respectively. Plot the curves of descending temperature and pressure as we pass outwards as before. The results may be tabulated thus :—

Temp. at p = 10,000.	Condensation	
	Pressure.	Temperature.
8000	7400	7200
10000	3850	7200
11850	2150	7050
14950	730	6250
21500	130	5250
500000	30 ?	4400

The table shows that if the initial pressure be far above the critical the result of increasing the initial temperature is to drive the photosphere into regions of diminished pressure. So long as

the pressure of condensation is above the critical, the temperature at which condensation begins remains unaltered; but as soon as this point is passed, and the curved part of the condensation line is reached, the temperature at which the clouds are formed begins to fall; that is to say, the effect of raising the internal temperature is to drive the photosphere further *out, and to cool it.*

Again, let us imagine that a temperature of 10,000° is found with pressures of 10000, 8000, 6000, 4000, 2000, and 200 atmospheres respectively, and tabulate the results as before.

Initial pressure $t = 10{,}000$.	Condensation	
	Pressure.	Temperature.
10000	3850	7200
8000	3050	7200
6000	2150	7050
4000	1150	6550
2000	420	5850
200	30 ?	4400

The sequence of changes is exactly similar, and the conclusion is that, if the initial temperature is constant, decrease of pressure will produce the same effect as increase of temperature.

Now the pressure at any point is determined by gravity and the mass of the superincumbent gases. It therefore appears that no carbon photosphere can have a higher temperature than the critical, and that the hotter photospheres must be those most deeply seated, or surrounding the most massive stars.

Deep-seated photospheres must mean heavy absorption, high photospheres little absorption.

Inspection of the diagram reveals a number of interesting points, and it is easy to see how the spectra of stars should be modified by altering the ratio of temperature and pressure. Call this $\frac{T}{P}$.

Let us at the outset give this expression a very high value, so high as to make the curve on the scale of our diagram raised very little above the line of no pressure. Such a star would have no photosphere. If the temperature were suitable small particles of incandescent carbon would be dispersed here and there in a thin mist at a high level. They would be unable to hide the radiation from gases at lower levels or to reverse the light from those among which they were spread. Such stars would be bright-line stars in which the lines would be mixed with a faint continuous spectrum which would begin with a dim radiance in the yellow-green.

As we reduce the ratio $\frac{T}{P}$ the dim radiance will spread and brighten, since condensation under higher pressure means a higher temperature. The mist stratum of the bright-line star

should then pass through denser stages into the condition of a discontinuous stratum of bright photospheric clouds still high up though lower than before. The bright clouds will be overlaid by those gases which lie highest, giving a spectrum showing the dark lines due to absorption. But with a discontinuous stratum of cloud there must be convection. Rising currents will make the clouds, descending currents the interspaces. The light, then, from the descending spaces should be bright lines corresponding with those which are reversed above the clouds.

If we now bear in mind the fact that a star spectrum is an integration of the whole light from the stellar surface, it is easy to see that the bright-line spectrum may be brighter than the continuous ; it may be equally bright or it may be less brilliant. If the convection movements were slow the respective results would be bright lines on a continuous background, continuous spectrum alone, or a simple absorption spectrum. It is, however, not likely that all the lines would behave alike, and the result should generally be a white or helium star showing some lines bright and others reversed.

If we may suppose the convection currents sufficiently rapid, then the bright lines due to descending currents should be shifted towards the red, while other bright lines from deeper layers might be undisturbed. We should then have some bright lines fringed on their more refrangible sides by dark companions.

Decreasing the ratio still further the spaces between the cloudlets will close up until we have the complete helium star.

Further progress will yield a yet brighter and hotter photosphere sinking step by step beneath stratum after stratum of gases. As the background gets brighter, helium absorption, having little intensity, becomes less and less obvious. Hydrogen absorption, on the contrary, becomes more and more extensive until it in turn becomes secondary to the absorption due to metallic vapours.

The curves showing the temperature and pressure at which condensation takes place indicate steadily rising pressure, and rising temperature (and, therefore, intrinsic brightness) until the critical point is reached. From this point no further change in $\frac{T}{P}$ can alter the temperature of the photosphere. As it sinks lower and lower the density of absorption increases. If the metallic-line spectrum of the Sun may be supposed to be formed under the conditions represented by the curve S, on passing to the next cooler line we might have the denser absorption of *Arcturus*.

If the ratio $\frac{T}{P}$ be still smaller we come to the curve A. Here the photosphere is still deeper. The outer parts of the superincumbent gases are much cooler, and compound vapours may be formed. If so, we should expect that the absorption should

consist of metallic lines, flutings, and general absorption of the kind known as smoke-veil; for instance, *α Orionis* and *Antares*.

Continuing the process, such a spectrum should grow in intensity until the light of the photosphere should be hidden by superincumbent vapours, or even non-luminous clouds formed by condensed metals and compounds. The last glimpse of the incandescent depths should be a dull-red glow.

Such, then, seems to be the normal history of a star.

There is, however, a special case.

Suppose $\frac{T}{P}$ is very high, but that its large value is due to extreme heat, and that P is itself large.

The result should be a very dense gaseous nucleus which should give a continuous spectrum, and therefore act as a deeper-seated photosphere whose light would be veiled by absorption in which that due to carbon vapour would be a conspicuous feature; but metallic lines would also be present, and if the absorption were great, or the intrinsic brightness of the continuous spectrum small, some of the strong metallic lines would stand out as bright lines—carbon stars. Such stars should pass through the normal sequence as a result of declining temperature, which may explain the former redness of *Sirius*.

It thus appears that bright-line white stars should be associated with nebulæ; that white stars being due to the greatest range of conditions should be most numerous, especially among the smaller stars; that solar stars should be next in order of frequency, and should form a larger proportion of the massive stars; that stars with fluted spectra should be comparatively few in number, and should as a rule be massive. Finally carbon stars, demanding exceptionally high pressure and temperature, should therefore be rare and vast.

There are several other deductions which may be drawn.

First, if a binary is formed by the fission of a single star. If the division is equal both stars should be white, or both solar, or beyond. If unequal and differing to a sufficient extent, the smaller would adhere to a Sirian spectrum long after the larger and less cooled had passed into the solar stage or beyond; as, for instance, *β Cygni*.

Secondly, any determination of the temperature of a photosphere is no guide to the temperature of the star-centre. Neither is the position of the photosphere as shown by the absorption much help. It is possible to have a large hot star showing exactly the same spectrum as a much smaller and cooler one. The ratio $\frac{T}{P}$ may be identical.

So far it has been assumed that carbon is the cause of all photospheres. It is of course possible that different stars may be differently constituted, and that different elements may play similar parts. But all the evidence of the spectroscope indicates a cosmic distribution of the elements best known to us. Moreover

a moment's thought will show that all that has been said in reference to a carbon photosphere will apply with equal force to any substance whatever. If, then, we can have photospheres formed of some heavier atoms, they should be situated deeper in the mass of the star, and should be overlaid by carbon, which should either form a higher photosphere in turn or should betray its presence by absorption. We ought, then, to have as great a variety of carbon stars as we have of other types. The fact of the rarity of carbon stars is one of the strongest evidences that it is pre-eminently *the* photospheric element. However this may be, the main conclusions here set forth remain unaffected, because they hold good for any substance which forms a photosphere, granting only the four postulates with which we started, and that this substance behaves like all others whose condensation-curves are known.

Observations of Comet a 1903 *from Photographs taken with the 30-inch Reflector of the Thompson Equatorial and the Astrographic Equatorial of the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

Date and G.M.T. 1903.				Apparent R.A.			Apparent Dec.			Corr. for Parl. R.A.	Corr. for Parl. Dec.	log Δ.
	d	h	m	s	h	m	s	°	′	″	s	″	
Jan.	28	5	54	31	23	7	51·38	4	30	20·5	+·17	+4·0	0·2130
Feb.	9	6	6	1	23	24	31·04	8	4	53·5	·20	4·1	0·1945
	9	6	19	59	23	24	31·93	8	5	2·7	·21	4·1	0·1945
	10	6	9	38	23	26	5·16	8	24	42·4	·21	4·1	0·1906
	10	6	24	14	23	26	6·61	8	24	59·6	·21	4·2	0·1906
	11	6	14	36	23	27	40·86	8	44	54·1	·21	4·2	0·1865
	11	6	25	21	23	27	41·67	8	45	4·0	·22	4·2	0·1865
	12	6	22	13	23	29	18·78	9	5	27·7	·22	4·2	0·1823
	17	6	24	41	23	37	50·76	10	51	55·8	·24	4·4	0·1587
	17	6	31	55	23	37	51·31	10	52	2·7	·24	4·4	0·1587
	18	6	22	16	23	39	38·30	11	13	58·2	·24	4·4	0·1534
	23	6	33	58	23	49	3·07	13	7	0·6	·27	4·8	0·1235
	25	6	33	46	23	52	59·49	13	52	30·8	·28	4·9	0·1098
	26	6	34	36	23	54	59·78	14	15	4·9	·29	5·0	0·1024
Mar.	3	6	35	24	0	5	11·79	16	0	47·3	·34	5·7	0·0439
	6	6	44	15	0	11	14·71	16	51	31·8	·35	5·9	0·0334
	11	6	53	4	0	20	12·23	17	23	25·6	·44	7·5	9·9348
	11	7	7	58	0	20	13·22	17	23	20·5	·45	7·6	9·9348
	11	7	25	59	0	20	14·34	17	23	17·6	·44	7·8	9·9348
	12	6	54	17	0	21	40·65	17	17	0·6	·46	7·8	9·9208
	13	6	55	55	0	23	0·06	17	4	44·4	·48	8·1	9·9048
	15	7	4	20	0	25	8·11	16	19	43·3	·51	8·9	9·8730
	16	6	59	24	0	25	54·72	15	45	48·7	+·53	+9·2	9·8570

The photographs up to and including the first on March 11 were taken with the reflector, the remainder with the Astrographic telescope. From January 28 to February 17 one exposure, and from February 18 to March 16 two exposures were given on each plate. Fuller details of these observations will be given in the Greenwich volume for 1904.

On the Astronomical Observations recorded in the "Nihongi," the ancient Chronicle of Japan. By E. B. Knobel.

Our knowledge of the *Nihongi*, which is the standard native history of ancient Japan, is derived from the translation made by Mr. W. G. Aston, which was published by the Japan Society a few years ago.

The *Nihongi* purports to be a history of Japan from the earliest mythological times to the year A.D. 697.

It contains the record of several astronomical observations, which are not without interest.

The observations consist of records of "Eclipses of the Sun and Moon," "Occultations of Stars and Planets," "Conjunction of Planets," "Apparition of Comets and Meteors," and two observations apparently of the Aurora Borealis.

The period covered by these observations is from the year A.D. 620 to 696.

The chronology of the earlier part of the *Nihongi* has been shown by several authorities to be entirely unreliable, and indeed Bramsen does not hesitate to assert that the fictitious dates therein ascribed to events, constitute one of the greatest literary frauds ever perpetrated. Though the above remark applies more particularly to chronicles before A.D. 600, it is of importance to critically examine the dates given to the astronomical events.

This is attended with some difficulty on account of the complicated chronological system in use in Japan, and which was borrowed entirely from China.

The elements of Japanese chronology are succinctly described by Mr. Kinoshita in his interesting little book, *Ancien Japon*, and also by the late Mr. Williams, the former assistant-secretary of the Royal Astronomical Society, in his work on "Chinese Observations of Comets"; but for the precise reconciliation of dates, the *Japanese Chronological Tables* of Bramsen, published at Tokio in 1880, though they go no further back than A.D. 645, are quite indispensable.

The year was a lunar year, consisting of twelve months of twenty-nine or thirty days; but in order to establish some correspondence between the months and the seasons, an additional month was intercalated every thirty-three months, or seven intercalary months in the lunar cycle of nineteen years. Common years had therefore 354 days, and embolismic years 383 or 384 days. The rules for determining the date of an intercalary

month are too complicated to be given here; but roughly, when the discrepancy between the civil and the solar years exceeds half a month, a correction is necessary, and an intercalary month is made. The months were designated by their ordinal numbers, but the intercalary month received the ordinal number of the preceding month. It is thus obvious that there is here a great possibility of error in reducing a date in Japanese or Chinese records to European chronology.

On this point the sexagenary cycle of years, of months, and of days, instituted by the Chinese in very ancient times, and continued by them and the Japanese to the present day, affords a means of checking dates with considerable certainty.

The commencement of the year was never coincident with January 1, but ranged from January 20 to February 18; the fact being that the year began with the new moon following the first new moon after the winter solstice.

The sexagenary cycle was formed by the combination of one series of twelve signs, named after the signs of the zodiac, called in Japanese Ju-ni-shi, and a second series of ten signs called Jikkan.

Sexagenary Table.

1. 甲子	21. 甲申	41. 甲辰
2. 乙丑	22. 乙酉	42. 乙巳
3. 丙寅	23. 丙戌	43. 丙午
4. 丁卯	24. 丁亥	44. 丁未
5. 戊辰	25. 戊子	45. 戊申
6. 己巳	26. 己丑	46. 己酉
7. 庚牛	27. 庚寅	47. 庚戌
8. 辛未	28. 辛卯	48. 辛亥
9. 壬申	29. 壬辰	49. 壬子
10. 癸酉	30. 癸巳	50. 癸丑
11. 甲戌	31. 甲午	51. 甲寅
12. 乙亥	32. 乙未	52. 乙卯
13. 丙子	33. 丙申	53. 丙辰
14. 丁丑	34. 丁酉	54. 丁巳
15. 戊寅	35. 戊戌	55. 戊午
16. 己卯	36. 己亥	56. 己未
17. 庚辰	37. 庚子	57. 庚申
18. 辛巳	38. 辛丑	58. 辛酉
19. 壬午	39. 壬寅	59. 壬戌
20. 癸未	40. 癸卯	60. 癸亥*

* It is preferable to use the Chinese characters rather than the words in Roman letters, on account of variations in orthography.

Months and days, like years, are counted by sexagenary cycles independently of each other.*

The Japanese call, under this system, the designation in the cycle the E-to † of the year, or month, or day. Intercalary months have no E-to. The E-to of the months for the Ju-ni-shi sign is always as follows :—

1st	month	Tora	寅	7th	month	Saru	申
2nd	,,	U	卯	8th	,,	Tori	酉
3rd	,,	Tatsu	辰	9th	,,	Inu	戌
4th	,,	Mi	巳	10th	,,	I	亥
5th	,,	Muma	午	11th	,,	Ne	子
6th	,,	Hitsuji	未	12th	,,	Ushi	丑

The following table from Bramsen's work may be useful :—

Perpetual Table showing the E-to of any Japanese Month.	If the Year from Jimmu-Tenno, B.C. 660, or the corresponding European Year, ends in				
	1 or 6.	2 or 7.	3 or 8.	4 or 9.	5 or 0.
1st month	27	39	51	3	15
2nd ,,	28	40	52	4	16
3rd ,,	29	41	53	5	17
4th ,,	30	42	54	6	18
5th ,,	31	43	55	7	19
6th ,,	32	44	56	8	20
7th ,,	33	45	57	9	21
8th ,,	34	46	58	10	22
9th ,,	35	47	59	11	23
10th ,,	36	48	60	12	24
11th ,,	37	49	1	13	25
12th ,,	38	50	2	14	26

* The Chinese sexagenary cycle of years was included by Ulug Beigh in his Samarcand Tables, 1437, and will be found also in the valuable chronological tables given by Johannes Gravius at the end of his *Epochæ Celebriores Ulug Beigi*, 1650.

† The name E-to is derived from the fact that the terminations of the Japanese names of the ten signs of Jikkan are alternately -e and -to, thus: Kinoe, Kinoto, Hinoe, Hinoto, &c.

The following are the astronomical records in order of subject :—

Eclipses of the Sun.

1. "36th year of Emperor Suiko, A.D. 628, spring, 3rd month, 2nd day. There was a total eclipse of the Sun."

The first day of the first month was February 10, and the second day of the third month was April 10. Oppolzer, in his *Canon der Finsternisse*, gives a total eclipse of the Sun, with the central line running through Japan, on April 10, $0^h\ 49^m{\cdot}7$ G.M.T.

2. "8th year of Emperor Jomei, A.D. 636, spring, 1st month, 1st day. There was an eclipse of the Sun."

The first day of the first month was February 12. There is no eclipse in Oppolzer's table which can possibly accord with this date.

3. "9th year of Emperor Jomei, A.D. 637, spring, 3rd month, 2nd day. There was an eclipse of the Sun."

The first day of the first month was February 1. The second day of the third month was April 2. Oppolzer gives a total eclipse of the Sun, with the central line running through Japan, April 1, $0^h\ 3^m$ G.M.T.—a discrepancy of one day.

4. "9th year of Emperor Temmu, A.D. 680, winter, 11th month, 1st day. There was an eclipse of the Sun."

The Shūkai quotes a statement that this eclipse was of $9\frac{1}{2}$ tenths, or nearly total." The first day of the eleventh month was November 27. Oppolzer's table gives an annular eclipse of the Sun, with the central line passing through Japan, on November 27, $3^h\ 13^m{\cdot}4$ G.M.T.

5. "10th year of Emperor Temmu, A.D. 681, winter, 10th month, 1st day. There was an eclipse of the Sun."

The first day of the tenth month was November 16. Oppolzer's table shows an annular eclipse of the Sun, with the central line passing through Southern China, on November 16, $2^h\ 24^m$ G.M.T.

6. "5th year of Emperor Jitō, A.D. 691, winter, 10th month, 1st day. There was an eclipse of the Sun."

The first day of the tenth month was October 27. Oppolzer's table gives an annular eclipse of the Sun, visible in Japan, October 27, $3^h\ 1^m{\cdot}9$ G.M.T.

7. "7th year of Emperor Jitō, A.D. 693, 3rd month, 1st day. There was an eclipse of the Sun."

The first day of the third month was April 11. Oppolzer gives an annular eclipse of the Sun April 11, $10^h\ 44^m$ G.M.T., visible only as a partial eclipse in Japan.

8. "7th year of Emperor Jitō, A.D. 693, 9th month, 1st day. There was an eclipse of the Sun."

The first day of the ninth month was October 5. Oppolzer gives a total eclipse of the Sun, with the central line running through South China, on October 5, $8^h\ 2^m{\cdot}1$ G.M.T.

9. "8th year of Emperor Jitō, A.D. 694, 3rd month, 1st day. There was an eclipse of the Sun."

The first day of the third month was March 31. Oppolzer gives a partial eclipse of the Sun March 31, $11^h\ 19^m{\cdot}7$ G.M.T.

10. "8th year of Emperor Jitō, A.D. 694, 9th month, 1st day. There was an eclipse of the Sun."

The first day of the ninth month was September 25. Oppolzer gives a partial eclipse of the Sun, September 24, $22^h\ 47^m{\cdot}3$ G.M.T. The correction for longitude makes the *Nihongi* date correct.

11. "10th year of Emperor Jitō, A.D. 696, autumn, 7th month, 1st day. There was an eclipse of the Sun."

The first day of the seventh month was August 4. Oppolzer gives an annular eclipse of the Sun, which could only be visible as a partial eclipse in Japan, August 3, $17^h\ 13^m{\cdot}6$ G.M.T. Here, again, the difference of longitude makes the *Nihongi* date correct.

Eclipses of the Moon.

1. "2nd year of the Emperor Kōgyoku, A.D. 643, 5th month, 16th day. There was an eclipse of the Moon."

The sixteenth day of the fifth month was June 8. In Oppolzer's table we find an eclipse of the Moon on June 7, $23^h\ 7^m$ G.M.T., which corrected for longitude agrees with the *Nihongi*.

2. "9th year of the Emperor Temmu, A.D. 680, 11th month, 16th day. There was an eclipse of the Moon."

The sixteenth day of the eleventh month was December 12. Oppolzer gives an eclipse of the Moon December 11, $14^h\ 8^m$ G.M.T., and the *Nihongi* date is correct.

Occultations.

1. "12th year of Emperor Jomei, A.D. 640, spring, 2nd month, 7th day. A star entered the Moon."

The seventh day of the second month is March 6.

2. "1st year of Emperor Kōgyoku, A.D. 642, autumn, 7th month, 9th day. A guest-star entered the Moon."

The ninth day of the seventh month is August 10.

3. "10th year of Emperor Temmu, A.D. 681, 9th month, 17th day. The planet *Mars* entered the Moon."

The seventeenth day of the ninth month is November 3.

Conjunction of Planets.

"6th year of Emperor Jitō, A.D. 692, autumn, 7th month, 28th day. On this night *Mars* and *Jupiter* approached and receded from one another four times in the room of one pace, alternately shining and disappearing."

The twenty-eighth day of the seventh month was September 14.*

* Very close conjunctions of *Mars* and *Jupiter* are recorded, A.D. 498, 509, 1170, 1591.

Comets.

"6th year of Emperor Jomei, A.D. 634, autumn, 8th month. A long star was seen in the south. The people of that time called it a besom-star. Hahaki-boshi or Hōki-boshi is the present name for a comet."

The eighth month began August 30. In Williams's *Comets observed in China* a comet is described in *Ma-twan-lin* as being seen, A.D. 634 September 22, in the constellation *Aquarius*, and which on October 3 was no longer visible.

"7th year of Emperor Jomei, A.D. 635, spring, 1st month. The besom-star went round and was seen in the East."

The date of the first day of the first month is January 24. This implies the reappearance of the 634 comet. It is not mentioned in the Chinese annals.

"11th year of Emperor Jomei, A.D. 639, 1st month, 26th day. A long star appeared in the north-west. Priest Bin said it was a besom-star. When it appeared there was famine."

The twenty-sixth day of the first month is March 6. The Chinese *Ma-twan-lin* records a comet, A.D. 639 April 30, as being seen in *Taurus*. Another Chinese chronicle makes the year 638, but neither is reconcilable with the *Nihongi*.

"5th year of Emperor Temmu, A.D. 676, autumn, 7th month. A star appeared in the East seven or eight feet in length. In the 9th month it at length disappeared from the sky."

The seventh month began August 15, and the ninth month October 13. Williams records from the Chinese annals that, A.D. 676, seventh moon, day Ting Hae = July 7, a comet was seen in *Gemini*. It was about three cubits in length. Its luminous envelope increased until it became thirty cubits in length. In the ninth moon, day Yih Yew = September 3, it disappeared. Clearly the same comet is referred to in both chronicles, and it is not improbable that the *Nihongi* account was copied from the Chinese. The discrepancy in the reduction of the date is due to Mr. Williams not having reckoned that the year 676 had an intercalary month, the first day of which was March 20.*

"10th year of Emperor Temmu, A.D. 681, 9th month, 16th day. A comet appeared."

The date is November 2. Probably taken from the Chinese, where it appeared, according to Williams, 681, ninth moon, day Ping Shin = October 17. The discrepancy is due to there being an intercalary month this year, which began August 20, and which Williams did not take into account.

"13th year of Temmu, A.D. 684, autumn, 7th month, 23rd day. A comet appeared in the north-west more than ten feet long."

* Some particulars of this comet are given by Sherburne in his work on *Manilius*, 1675. He states that it appeared in August, and lasted three months.

The 23rd day of the seventh month was September 7, there having been an intercalary month this year, the first day of which was May 20. The Chinese record, given by Williams, states: "In the epoch Wan Ming, 1st year, 7th moon, day Sin Wei, there was a comet in the West. It was about ten cubits in length. In the 8th moon, day Kea Shin, it disappeared." From this Williams gives 684 July 8 as the date when the comet was seen, and August 10 as the date of its disappearance. He further remarks that Biot makes the dates September 6 and October 9, but by computation it comes out as he has rendered it. There can be no doubt that this is Halley's Comet. The perihelion passage, according to Hind, was A.D. 684·80, which gives the date September 18.

It is impossible to reconcile Williams's dates for this comet. There can be little doubt, from the Chinese and Japanese descriptions, that the same comet is referred to. A.D. 684 was the first year of the lunar cycle, and it was embolistic. The intercalary moon began on May 20, and it followed the fourth moon of the year; consequently the seventh moon of the chronicle was actually the eighth moon of the year. I append the dates of the first day of each month in the year in question:—

1st month, 1st day	January 23
2nd ,, ,, ...	February 21
3rd ,, ,, ...	March 22
4th ,, ,, ...	April 20
Intercalary 4th month, 1st day ...	May 20
5th month, 1st day	June 18
6th ,, ,, ...	July 18
7th ,, ,, ...	August 16
8th ,, ,, ...	September 15
9th ,, ,, ...	October 14
10th ,, ,, ...	November 13
11th ,, ,, ...	December 12
12th ,, ,, ...	January 11 (685)

Meteors.

"9th year of Emperor Jomei, A.D. 637, spring, 2nd month, 23rd day. A great star floated from East to West, and there was a noise like that of thunder. The people of that day said it was the sound of the falling star."

The date is A.D. 637 March 24.

"3rd year of Emperor Tenchi, A.D. 664, 3rd month. There was a star which fell north of the capital."

The date is A.D. 664 April.

"11th year of Emperor Temmu, A.D. 682, 8th month, 3rd day. On this evening, at twilight, a great star passed from the East to the West."

The date is A.D. 682 September 10.

"13th year of Emperor Temmu, A.D. 684, 11th month, 23rd day. At sunset a star fell in the quarter of the east as large as a jar. At the hour of the Dog (*id est* 7 to 9 P.M.) the constellations were wholly disordered and stars fell like rain. During this month there was a star which shot up in the zenith and proceeded along with the *Pleiades* until the end of the month, when it disappeared."

This would seem to be a comet. The date is A.D. 685 January 3.

Aurora Borealis.

"28th year of Emperor Suiko, A.D. 620, 12th month, 1st day. There was a red appearance in the sky, over a rod in length, and resembling the tail of a fowl in shape."

Date A.D. 620 December 31.

"9th year of Emperor Temmu, A.D. 680, 11th month, 3rd day. There was a brightness in the east from the hour of the Dog to the hour of the Rat (*id est* 8 P.M. to midnight)."

Date A.D. 680 November 29. The hour of the Dog 戌 was from 7 P.M. to 9 P.M., and the hour of the Rat 子 from 11 P.M. to 1 A.M.*

* The twelve signs of the Ju-ni-shi were used to designate the twelve Chinese hours of the day, each of which consists of 120 minutes.